DEPARTMENT OF APPLIED STATISTICS
UNIVERSITY COLLEGE, LONDON.

Questions of the Day and of the Fray

No. VI.

EUGENICS AND PUBLIC HEALTH

[A LECTURE DELIVERED AT THE YORK CONGRESS OF THE ROYAL SANITARY INSTITUTE, JULY 30TH, 1912]

BY

KARL PEARSON, F.R.S.
GALTON PROFESSOR OF EUGENICS

WITH SIX DIAGRAMS IN THE TEXT

LONDON
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EUGENICS AND PUBLIC HEALTH

AN ADDRESS

TO PUBLIC HEALTH OFFICERS

[BEING A LECTURE DELIVERED AT THE YORK CONGRESS OF THE ROYAL SANITARY INSTITUTE, JULY 30, 1912]

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UNIVERSITY OF LONDON

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[Delivered at the Congress of The Royal Sanitary Institute at York, July 29th to August 3rd, 1912.]

CONGRESS AT YORK.

LECTURE TO THE CONGRESS

By KARL PEARSON, F.R.S.,

Galton Professor of Eugenics, University of London.

EUGENICS AND PUBLIC HEALTH.

IT is with very great hesitation that I stand on this platform to-night before an audience, many of whom are experts in public health matters, and several of whom have for years been first-hand workers at social problems. It requires some courage, I assure you, to come before you and say that having no training in public health, that having no medical knowledge, and that having only second-hand acquaintance with social problems, I still feel that I have a mission, a gospel to preach to those who have. There are those (very likely represented in my audience to-night) who feel, I know, strongly that a mere mathematician, a figure-twisting statistician, has no business whatever in this field. Well, I will frankly proclaim that I am no believer whatever in the water-tight compartment theory of human knowledge. That theory is opposed to all the history of scientific progress. Science has progressed just as far as new methods and new ideas came into any branch of it, and in nine cases out of ten those methods and ideas have come in from outside, from what appeared to be at the time quite foreign divisions of knowledge. Shall I give you illustrations? What had the astronomers made of their science before the advent of mathematics? Mere astrology. Then came Kepler, Newton and Laplace, and astronomy became the most certain of all sciences. What part did electricity play in life, municipally, socially or medically sixty years ago? Then came the

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mathematical physicists, Clerk-Maxwell, Lord Kelvin, Hertz, Röntgen, and lo! our very habits of life and treatment were revolutionised.

And to take an example nearer home. What was the science of medicine in 1840? Surely you will agree with me when I say, it was an empirical science based upon clinical observation, and often largely upon unjustified if not unjustifiable opinion. What has differentiated modern medicine from early Victorian medicine? Is it wholly knowledge gained at the bedside? Or is the advance due to the pressure of new methods and new ideas breaking in from biology and from physics in their broad senses? Is it not the laboratory work of the bacteriologist, of the physiologist, of the parasitologist, and even of the physicist which has revolutionised modern medicine? May it not well be that at least one great branch of medicine, the public health service, has yet room for further new methods and new ideas, and that some of these may come from the mathematical statistician? Is there not room, nay need, for the medical mathematician in the field of public health?

The name at least *ἰατρο-μᾰθημᾰτικός*, medical mathematician, is Greek or rather Egyptian in origin, if to our generation belong the first attempts to make mathematics a really serviceable handmaid of medicine. In saying this I am perhaps overlooking an older link between medicine and mathematics. I refer to the relation between medicine and the actuarial profession: the actuary is only a specialised mathematician; but here our positions are rather reversed, for the science of medicine is the handmaid of insurance; it is the mathematician in that case who unties the purse strings, and I am speaking rather of the day when the medical public health service will directly seek the aid of the specialised mathematician.

That is the point I want to illustrate to you to-night, that is the mission which leads me to this platform, and I think that must be the real ground upon which the Council of The Royal Sanitary Institute requested me to lecture to this Congress. I have come that I may endeavour to show you that in the field of public health, in the treatment of social welfare, an *entente cordiale* of mathematics and medicine is of no less importance than another partnership in the diplomatic sphere.

I must ask you to be patient with me while I endeavour to explain my views, which, for the moment, may seem to touch only one side of the title of my lecture. In the first place many of you are aware of an enormous literature (if I may venture to call it such) which has sprung up during the last ten years. I refer to the annual reports of the medical officers of health and of the school medical officers. You are also aware of the still more gigantic manuscript material, the hundreds of thousands of schedules

which lie behind this literature. I am convinced that some of you are asking yourself the *cui bono* of this great national record, the very storage of which is becoming a "burning" question in the smaller districts.

Well, those members of my audience who are medical officers of health have certainly read their own reports, but how far have they had the leisure to study those of their colleagues? How far again have they been able to compare their schedules and records with those of other districts, scattered by the hundred up and down this country? By comparison of schedules, I do not mean the comparison of printed forms. It is quite easy to draft a most all-embracing questionnaire; I refer to the accuracy and comprehensiveness with which the records themselves are kept, and how far the questions asked are answerable, having regard to the education of the questioned, the sympathy and tact of the questioner, and the practical limits to official cross-examination and to human life. I am, Ladies and Gentlemen, a great reader of the literature I have referred to; I am by no means certain that I do not know more of the contents of these medical officers' reports than is known to any single medical officer. I welcome, study and *file* every such report that the courtesy of the public health officers (now in increasing numbers) sends to my laboratory. I have spoken to you of the *entente cordiale* of medicine and mathematics as a thing of the future, yet I venture to believe it is almost a fact of the present. From the Midlands to Scotland, from the great manufacturing towns of Yorkshire and Lancashire, thousands of the schedules to which I have referred have reached my laboratory through the kindness of the officers of the public health service. And (in case any of those who have loaned them are among my audience) I would appeal for patience and time. I see work for a staff of fifty trained computers, where I have to work with one of five. I would express my deep gratitude to these medical officers and say that we are toiling to the conclusion of a mighty task. But what we have learned from close examination of many schedules from many counties is this:

First.—An elaborate questionnaire is of no service at all, if either 50 to 70 per cent. of the entries are not filled in, or filled in parrot-fashion. Let me illustrate exactly what I mean. In some recent inquiries as to infantile mortality there was a column for recording when the father had "bad health." In order that this entry should be really useful some broad definitions ought to be given of health depending upon weeks of sickness, absence from work, hospital attendance, and so forth. For my own laboratory work we have issued the following scheme, but it might be easily bettered or modified to suit special inquiries:—

HEALTH (ADULT, MALE OR FEMALE).

Health should be judged with reference to two standards based on the answers to the questions:—

(*a*) How often has it been necessary for the subject to consult a doctor, or seek advice or treatment at a hospital?

(*b*) How often has he been unable to work through illness?

SCALE.

V.R. Very robust.—He has never had to see a doctor, has never been in hospital, nor been off work through illness.

R. Robust.—He has only seen a doctor about minor ailments, and has only been off work for colds, etc.

N.H. Normally healthy.—He has not had more than one serious illness, involving, say, a fortnight's absence from work during the last ten years.

R.D. Rather delicate.—He has had more than one serious illness, but not more than one involving more than four weeks' absence from work during the last ten years.

D. Delicate.—He is off work through illness at least four weeks in all every year.

V.D. Very Delicate.—He is in a chronic state of ill-health.

NOTE.—Serious accidents should be noted, and it might be asked how often he has received money from a sick club, if a member.

What can possibly be learnt when the column of the health of the father is left blank in 90 per cent. of cases, and there is no knowledge whether the recorder was unable to ascertain it, or whether in all these instances the father was in uniform good health?

I think again of nearly 8,000 records from one town I have seen of infantile mortality, where there is a column for the mother's health, and in only 3 per cent. or 4 per cent. of these cases is she said to have had other than good health! I wish indeed it were a fact, but I know from similar towns where the health of the mother has been really carefully investigated, that there is a fairly intimate relation between this health and the survival of the infant. Indeed, if you look with the eye of a statistician on these schedules, and apply tests well known in the profession, you can put your finger at once on the men whose records are to be trusted, and whose staff are enthusiastic in their work. Nay, more, one can very often say within a given administrative district, which assistant-officers, nurses, and visitors are capable and conscientious, and which are filling in their schedules mechanically and superficially. I had quite recently to return a long series of schedules dealing with all the physically

and mentally defective children of a large northern town, which had been most kindly placed at my disposal, because, after a thorough examination of them, it was clear from internal evidence that they had been filled in without due inquiry and safe controls.

Secondly.—You may have the most conscientious district visitors, the most energetic staff of school medical officers, but if nothing has been done to standardise their judgments, the result will be hopeless confusion. I know from the schedules how the most conscientious assistant school medical officers, how the most enthusiastic and careful district visitors, differ in what we term their *personal equation.* One visitor finds 8 per cent. of a certain characteristic, another working in an exactly similar or the same district finds 25 per cent. Even more anomalous results may be found in the school medical inspection returns. Yet in nine cases out of ten records are pooled without regard to this vital question of personal equation. Masses of material are indeed useless because no real attention has been paid to this matter. There are methods of correcting for personal equation, there is nothing new in the problem, astronomers have felt the difficulty and met it for more than a century. But it is the application of an old theory to a new field, and if 10 per cent. of public health officers have realised the difficulty, scarcely 1 per cent. have attempted to meet it.

With descriptive and qualitative characters, such as must be largely used in work of this kind, absolute personal equation is of no importance, relative personal equation is everything; and far more comprehensive methods are needed for standardising the public health staff of each district, and eventually for comparing the staff of one district with a second. The standard man of the district ought to be compared with each new comer, by letting the pair record independently the same 50 or 100 cases, and then from the results determining the exact nature of their personal equation either with a view to correcting it on actual observations or by an attempt to modify the new comer's methods of judgment. Probably, as research in other fields has shown, the former is the safer plan, but each local officer will have to consider as a matter of primary importance this standardisation of his staff, and ultimately, if we are to draw conclusions as a whole for the relative conditions of different districts, there will have to be a comparison of the standard men of these districts. What value have the returns for defective teeth or eyesight for different districts for comparative study of occupations and environments, when we know that the mere introduction of a trained ophthalmologist or a dental surgeon has sufficed in a given district or school to increase the reported defective-

ness by 100 per cent. to 200 per cent. of its value? What reason have we for supposing that the prevalence among school children of mental defect or of tuberculosis can really be estimated for the different districts by the percentages returned to the Board of Education? Until some effective system of interdistrict standardisation is called into existence, it is more reasonable to believe that personal equation, which we know to vary enormously, rather than local conditions may be the source of these percentage differences. Unless the public health service proceeds in this matter with great caution and in the true scientific spirit, we may have the normal child of one parish segregated as feeble-minded or defective when it crosses the borders into a second parish. We are almost certain to have widely different standards for notification of the tuberculous in different districts, and I anticipate most wonderful publications on the influence of climate and environment on the prevalency of tuberculosis as a result!

To those medical officers who have to direct a large staff of assistant officers and district visitors, I would therefore emphasise, first, the idleness of the records, if they are filled in without keen insight and enthusiasm. All categories must be adequately defined and given, if feasible, quantitative value, and, further, the staff must appreciate the importance of caution in treating individual cases. This is our Scylla. But close alongside stands our Charybdis, the wreck-bestrewn rock of unregarded personal equation. I feel convinced that the public health service can only steer this course under mathematical pilotage, and even then, were a veritable Ulysses at the helm, there would still be a toll of failure. The tests for slovenly record, the tests for differentiated records, and the means for correcting the latter are essentially statistical and mathematical problems, and the most valuable labour will be wasted until this is recognised.

But Scylla and Charybdis are not the only rocks in this newly-discovered and largely uncharted sea. The value to be given to weight of numbers is at present wholly unappreciated by many of the captains who are seeking to traverse it. I should like to illustrate this point in a variety of cases.

For example, quite recently a distinguished investigator came to me troubled in mind over contradictory results. The English workers had tested twenty-eight cases of tuberculosis and found bacilli of the bovine type in two. But the Germans had then tested one hundred cases and found bacilli of bovine type in *none*. How many cases must be investigated in order to settle the true percentage? That is a very nice mathematical question, even if we can assume that the above-referred-to

Scylla and Charybdis were already cleared, which I, individually, am by no means certain was the fact.

Again, to come nearer home, a distinguished assistant medical officer, Dr. Ewart, of Middlesbrough, has apparently felt himself able on *seventy-nine* cases of tuberculosis to determine the influence of mother's age at birth of child on the liability of the child to phthisis. (*Public Health*, May, 1912, p. 312. "Problems of Race.") He tells us, on the basis of his seventy-nine cases, that "this disease falls at the beginning and with increasing incidence on those born later in life." He suggests, if I follow him, that the fall in the phthisis death-rate is due to the limitation of families, so that the children born later in the life of the mother are no longer born in the same numbers. He, however, gives no comparative evidence of the distribution of children at age of birth of mother now and thirty and more years ago. In the bulk of the population there is little difference between the age of marriage now and fifty years ago:

TABLE 1.—*Average Age of Wives at Marriage in England.*

1851	.....	25·88	1890	.....	26·03
1860	.....	25·83	1900	.....	26·24
1870	.....	25·71	1907	.....	26·45
1880	.....	25·72	1909	.....	26·65

The total change is under a year, and would not carry the mean wife outside one of Dr. Ewart's groups into a second. Further, until the present census returns are published, we have no real evidence that it is the late children who are being dispensed with, and not an increasing interval between births being allowed as the basis of limitation. Here, however, are the seventy-nine cases on which Dr. Ewart has based his theory. You will notice how the tuberculous are in excess among the earlier and later born:

TABLE 2.

Age of Mother.	Phthisis.	Per Mille Phthisical.	Per Mille Normal.
20 and under	8	101	67
21—25	15	190	256
26—30	16	203	289
31—35	17	215	203
36—40	14	178	147
41 and over	9	113	38
	79	1,000	1,000

In the comparison of the 3rd and 4th columns Dr. Ewart finds the great objection to be that the tuberculous data were gathered from South-East Lancashire and the normal population from the North Riding of Yorkshire. I presume he means Middlesbrough, which is in every respect differentiated from the dale population of the North Riding. There is not the least doubt that the objection is a valid one. I have data for the age of mother at birth of offspring from many districts, agricultural and urban; they vary widely, you may find any number from 3 per cent. to 9 per cent. of offspring born of mothers over 40 years of age, and any number from 2 per cent. to 7 per cent. of offspring born of mothers under 21 years of age according to the district. Nevertheless, this is not the *great* objection to Dr. Ewart's result; the real objection is the paucity of cases he has ventured to use in drawing a sweeping conclusion. How many cases ought we to use? At least a thousand and better four thousand, and we ought to compare them against normal material drawn from the same districts. I cannot make any such valid investigation, for at present I have only 400 cases of tuberculosis with age of mother at birth. Of these 100 are drawn from pedigree data in my laboratory, 140 from a sanatorium in the north of England, 160 from the Brompton Hospital in London by the kindness of Drs. Batty-Shaw and Bernstein. Against these I set a normal population based upon something like 50,000 cases drawn from London, Northumberland, Lancashire and Yorkshire in fairly appropriate proportions. The results are given below:

TABLE 3.

Mothers' Age.	Phthisical Population		General Population.	
20 and under	56		51	
21—25	224		219	
26—30	308		303	
31—35	211		224	
36—40	158	201	149	203
41 and over	43		54	
	1,000		1,000	

You will see that the younger mothers have a slight excess of tuberculous children, as Dr. Ewart found, but the older mothers a slight deficiency, contrary to what he found. The former result may be a secondary effect of a rule I have elsewhere stated, that the elder or firstborn children are more liable to tuberculosis, and such children come on the average from younger mothers. But are such differences in this case really significant?

You all know perfectly well that if you tossed a shilling a hundred times and counted the heads and tails, you would be very unlikely to get 50 heads and 50 tails; but if you got 35 heads and 65 tails, what would be the significance of that? What is the probability that it is a mere chance result, or is the coin actually loaded? Well, a mathematician would tell you that you might expect that result once in about 1,000 trials, and accordingly it is the more reasonable supposition that, having got it on a single trial, the coin was biassed. Is that not exactly the same type of problem as we have in our tuberculosis data? We have taken 400 cases out of a general population, and we must not expect to get them in exactly the same proportions of mothers' ages at birth as they occur in the general population. Now this can be illustrated quite simply. Put 1,000 counters into a bag, 51 yellow to represent the mothers of 20 and under, 219 blue to represent the mothers 21—25, 303 red to represent the mothers 26—30, and so on; then proceed to draw a sample population from this bag, one by one, returning each counter as drawn. Here are some results:—

TABLE 4.—*Per Mille.*

Age of Mother.	Actual Pop.	Samples of 79.		Samples of 400.	
		1st Exp.	2nd Exp.	1st Exp.	2nd Exp.
20 and under	51	26	51	60	87
21—25	219	329	215	235	255
26—30	303	253	278	283	253
31—35	224	177	228	230	215
36—40	149	139	190	115	150
41 and over	54	76	38	77	40

From the first sample of 79 you would suppose a defect of young mothers and an excess of old mothers; from the second a defect of old mothers. Even from the 400 samples you might draw all sorts of startling conclusions! The public health officer should toss coins, calculate the probable number of trumps in his partner's hand at whist, and test generally how far random samples deviate from the expected, before he ventures to assert that his data indicate great scientific laws!

It is, I fear, only the trained statistician, however, who will be able to tell us whether the deviations are the result of random sampling or represent true bias. Into that theory I cannot enter on this occasion, it is part of the training which the *ἰατρο-μᾰθημᾰτικός* of the future will seek and obtain. But one method which shows in rough and ready way the result is to plot a certain quantity termed the probable error on either side of the expected, and question to what extent the compared series falls outside this belt.

In the accompanying diagram this is done. Had the observed phthisical series gone *three* times outside this belt we could not have asserted the result to be significant. It goes once slightly outside, and that is no

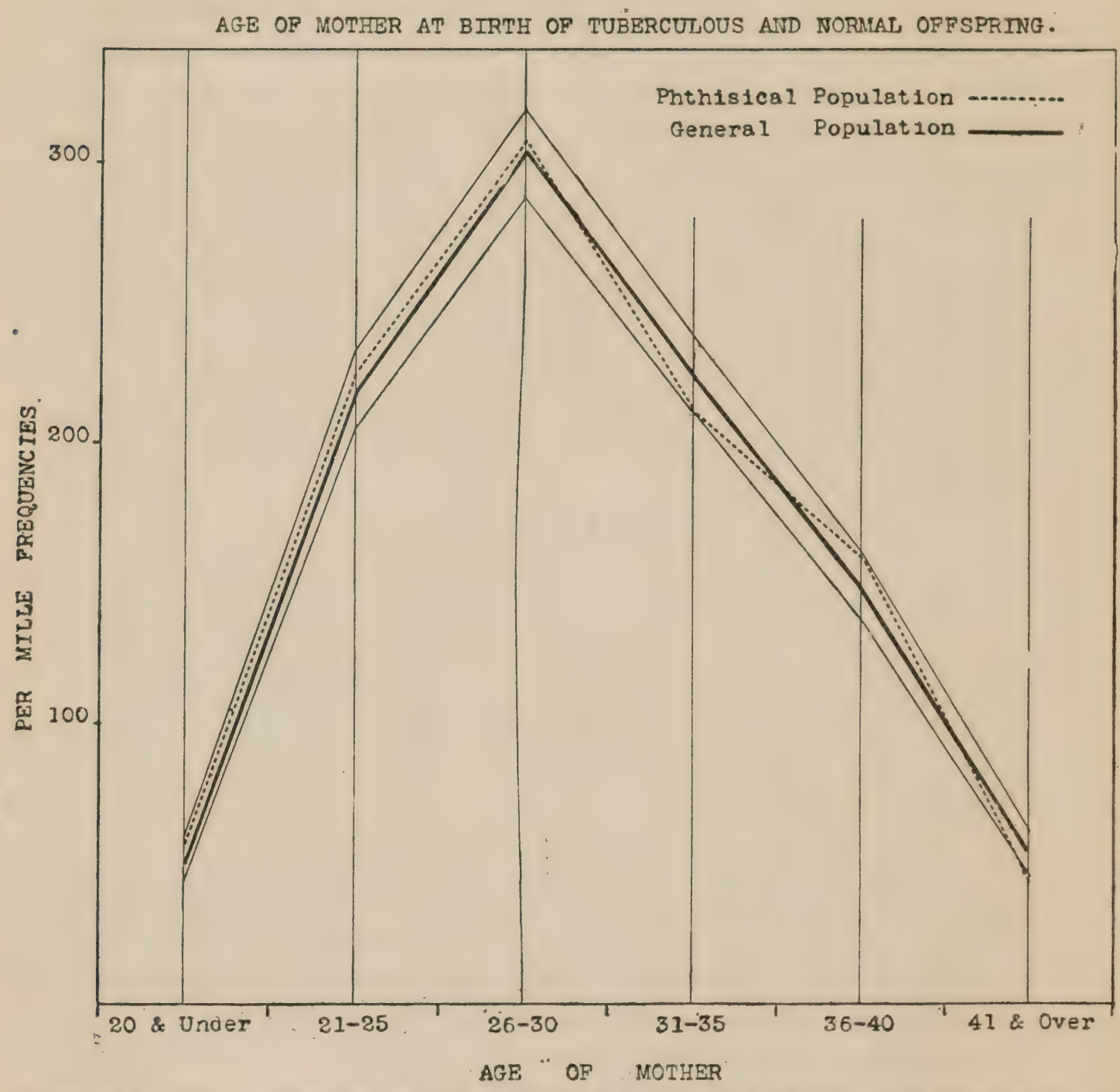

Diagram I.—To show there is no significant influence of age of mother at birth of child on the chance of child becoming tuberculous.

significant deviation. Hence on the basis of five times Dr. Ewart's numbers I am not able to assert that the age at birth of the mother has any influence whatever on the chance of the child being tuberculous.

Now please remember that I do not state that no such relation exists. I merely say that we can at present draw no inference at all, and that

there is grave danger when medical officers of health proceed on inadequate data to draw very sweeping conclusions by wholly unscientific methods. Until the complete *ἰατρο-μᾰθημᾰτικός* is trained to the public health service most astonishing results will be reached unless medicine and mathematics go hand in hand.

I must illustrate this neglect of numbers once more. I cite from the statement of a medical officer of health published by the London Eugenics Education Society, a society which professes to help forward the cause of Eugenics.* He writes:

> "The influence, moreover, of one birth on the sex of the next, and the relationship of the sex on the interval of time to the next impregnation, show that the substance derived from the fœtus persist after birth and influence the succeeding one."

Now here is a direct statement† that if parents have a boy at one birth, it will influence the sex of the next child born to them. Now let us look exactly at the data upon which this result is based. The author took 382 births and found:

TABLE 5.

	Boy following Boy.	Girl following Boy.	Boy following Girl.	Girl following Girl.
Cases	90	102	100	90
Average Interval... ...	2·9 years	2·6 years	3·0 years	2·8 years
and again:				
Sequence in Sex	180 times.			
Change of Sex	202 times.			

Now no attempt is made to ascertain whether these differences are *statistically* significant, but a sweeping scientific theory is based upon them, that substances derived from the fœtus persist after birth and influence the sex of the next child!

What is the likelihood that in taking samples of 100 cases out of material of which the average is 2·8, we shall get such deviations by pure chance as 2·6 and 3·0? The author has given us no means of answering it, but it involves the variability of the interval between two births of which the average is 2·8 years, being something between a year and a year and a half. This is so plausible that I have no hesitation in saying that the observed differences carry no weight at all; they are due simply to paucity of material. Look again at the second result. What should we expect in 382 births? Why, that there would be change of sex in 191 cases. It occurred in 202 cases, or 11 times in excess of what would

* R. J. Ewart. "The Aristocracy of Infancy and the Conditions of its Birth." *The Eugenics Review*. July 1911.

† This paper abounds in similar inadequately demonstrated theses.

happen on a very long experience. Are those 11 cases significant or not? Our medical officer of health assumes them to be so without any inquiry! What is the problem? Why, the chance of a sequence is nearly one-half. Therefore toss a coin 382 times and ask yourself whether 202 heads instead of 191 would mean that the coin was biassed. Why, the mathematician will tell you that once in every three to four trials of such an experiment the number of heads would differ from the theoretical value

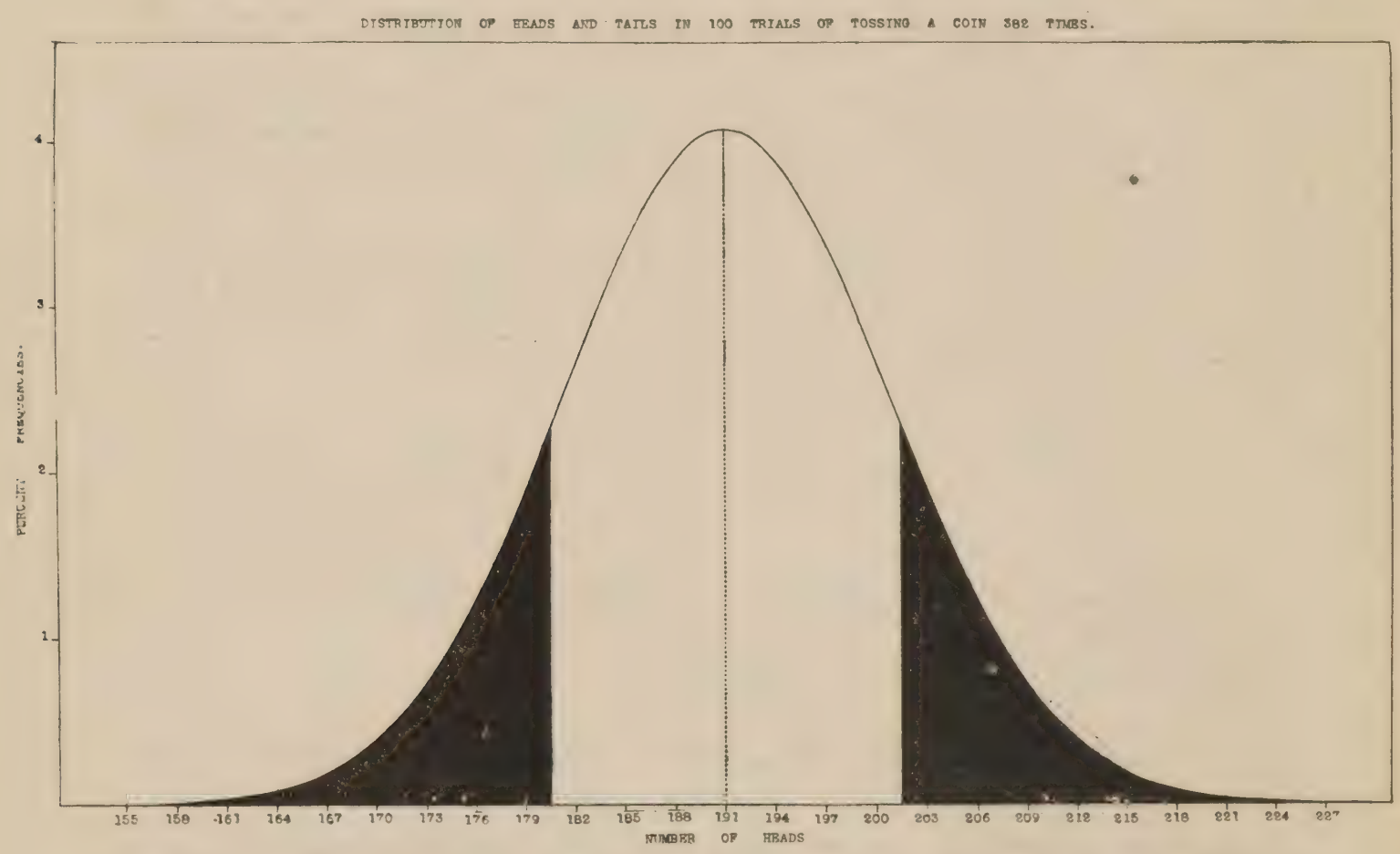

Diagram II.—The black area relative to white shows the odds against a deviation of 11 heads or tails from the theoretical number 191 in throwing 382 coins.

191 by 11 or more units. In order to illustrate this point eight experiments of tossing a coin 382 times were made, and the following results were recorded:

TABLE 6.

	Heads.	Tails.
1st experiment	188	194
2nd ,,	192	190
3rd ,,	187	195
4th ,,	204	178
5th ,,	184	198
6th ,,	186	196
7th ,,	205	177
8th ,,	192	190
Experiment with sex of babies	202	180
Theory	191	191

No legitimate conclusion whatever can be drawn from such data. Whether the theory that a previous birth influences a later birth be true or not, it cannot be answered by such inadequate data and inadequate statistical methods. They can only harm the science that Eugenists and the Public Health Service alike wish to forward.

I could multiply such cases a hundredfold, they occur over and over again when the medical officer of health endeavours to draw conclusions from his data, without any training in the modern theory of statistics. I do not cite such errors in a spirit of criticism or of anything but sadness over such blunders. I feel that the science of eugenics—the study of what improves or impairs racial efficiency—must go hand in hand with the science of public health. Whatever makes for adequate treatment of public health problems, makes for sure foundations for the science of eugenics; whatever deductions are drawn by superficial and inadequate treatment from public health experience will damage the science of eugenics as much as they damage the science of public health. It is almost impossible to regret too keenly the hasty generalisations which are being formed both in your science and in my own. No science can be built up by popular discussions before its fundamental principles are firmly based. To be a true science it must have developed appropriate methods, have found its raw material and established a school of trained workers, who are devoting their lives to its pursuit. Can that be said of eugenics at the present time? I sadly fear it cannot.

If we sum up the situation, we cannot deny that the material is ample; to a very large extent, although, of course, not wholly, the public health service is producing it. That material is essentially statistical, and as such must be dealt with by the methods of modern statistics. But these methods cannot be learnt and applied, any more than the differential calculus can be, without adequate training. I look forward to the time when every great municipality in this country will have its trained statistician, and, what is more important still, when there will be a Government Statistical Bureau to supervise the reports of the chief state Statistical Departments such as those of the Local Government Board, the Education Department, the Registrar General's Department, and the Home Office. Almost every continental government has now such a bureau; only Great Britain, where practically modern statistics have been created, lacks such an essential executive department. How can we expect municipal authorities to appreciate adequate treatment of their local data, when our chief government officials are among the gravest offenders in this respect?

Please do not think I am exaggerating the state of affairs. I could

illustrate the evil at every turn. It will suffice to do so in one or two cases. There are few questions of more importance from the public health standpoint than the origin of cancer. You know that it is a widely spread impression that cancer is associated with certain houses; the idea is that certain houses (the so-called "cancer houses") receive more than their due number of cancer cases. The conception is an important one, and, if it were ever established, it would not only throw light on cancer, but would open up a new field for the activities of the public health officer. Well, how is such a problem to be answered? Let us suppose a number of small compartments placed on a board, the number of these compartments being equal to the number of houses in a given district, and the size of these compartment proportional to the average number of occupants of the houses. Now let a number of marbles be taken equal to the total number of cancer cases which have occurred during a given number of years in the district. Let these marbles be thrown at random into the compartments. In some compartments will be found one, in some two, and in some three or more marbles. If cancer cases have no relation to houses, and we put aside the question of occupational mortality,* then the compartments with several marbles will represent the distribution of multiple cancer houses. The problem in a limited and simplified form can be solved mathematically. If the actual number of houses with multiple cases of cancer far exceeds those determined by our chance distribution, then we should begin to believe that so-called "cancer houses" were a reality. There will, of course, always be houses with multiple cancer cases in any district; the real question is: Do they exceed those which we should expect would arise from a random distribution of our marbles? To answer this even approximately we must know at least the *total* number of cancer cases and the total number of houses in which they occurred, classified according to their average size of households. Now only a few years ago a Government report on cancer was issued by the Registrar General for Ireland, and the author deals among other things with the problem of "cancer houses." He had before him of necessity the total number of cancer cases of the City of Dublin for ten years; he must have known the total number of inhabited houses in the City and presumably the population. Yet all he tells us is that in twelve houses†

* The multiple houses being determined in a district, it is of importance to ascertain whether the cases in the same house belonged to (1) the same family; (2) followed the same occupation.

† On any reasonable supposition as to number of cases, the multiple houses in Dublin seem at least 50 per cent. less than the chance number, and we should be compelled to suppose, if the data were worth anything, that the appearance of one case of cancer conferred immunity on a house!

two cases of cancer occurred in those years. There is no statement as to whether, considering the number of houses, the number of inhabitants, and the number of cases of cancer, such a result would be likely to arise from pure chance; he does not tell us how many houses had three or more cases, only that there were twelve houses with two cases. From this he solemnly draws the conclusion that:

> "In some instances more than one case of cancer has occurred amongst different families living in the same house, or amongst successive occupants of the same house." (Report p. 39.)

By the General Registry for Ireland I am told that it is *now* impossible to ascertain the number of cancer cases that occurred in the City of Dublin for those years, yet that number must have been before the author of the Report!

In a similar manner the author illustrates the bearing of alcoholism and syphilis on cancer; he tells us that there were so many cases in which cancer was associated in the same person with one or other of these diseases, but there is not a word as to the frequency with which these diseases occur in the City of Dublin! Now, Ladies and Gentlemen, I feel sure you would simply smile if I told you that I had known 18 cases of cancer associated with dark hair, and 10 cases of cancer associated with Roman noses, without informing you of the total percentages of dark-haired individuals and of Roman-nosed persons exposed to risk! Yet that sort of information is precisely what this Government Report on cancer provides us with. You may consider that this is an exceptional case, but I assure you it is not. I could cite equally fatuous argumentations from the Local Government Board Reports, the Reports of the Prison Commissioners, or the Reports of the Royal Commissions and Departmental Committees of both this country and of Scotland; but time does not permit, and my sole object is to emphasize how very important in these matters a thorough statistical training must be. Throughout the country immense masses of data are being collected by the public health and school medical services, the conclusions which may be drawn from them, if sound, will be of the highest national importance; they touch the very problems with which eugenics has to deal, the consideration of those agencies under social control which may improve or impair the racial qualities of future generations. But if Government officials themselves treat these problems superficially, who shall set a standard to the public health service? If I can but make this audience realise the urgent need for a Government Statistical Bureau to deal with problems of public health, I shall have more than fulfilled my purpose to-night of aiding both your science and my own.

Thus far I have tried to illustrate the dangers which flow from neglect of personal equation, from careless observation, or from paucity of data. But, given careful observation and ample data, a new series of difficulties arises of an even more subtle kind. I shall indicate their nature best by an example taken from the recent excellent report of Dr. Chalmers of Glasgow, on the Administrative Treatment of Pulmonary Phthisis for 1910, the first year of compulsory notification in that city. By the courtesy of Dr. Chalmers, I am able to add the notifications for 1911, making 4,413 cases in all, and to use the results of the 1911 Census. We then have the following table :—

TABLE 7.—*Housing of the Consumptive.*

	Number of rooms.			
	1	2	3	4 and over.
Percentage (Census, 1911) of population occupying houses of	13·8	48·3	21·1	16·8
Percentage of 4,413 notifications of phthisis occurring in houses of (1910 and 1911).....	22·3	53·6	15·5	8·6
	+ 13·8 %		− 13·8 %	

Now surely there is not the least doubt on the basis of these figures of an association between phthisis and the size of the tenement? Dr. Chalmers wisely draws no inference (at any rate at this part of his Report), but leaves the figures to speak for themselves. I feel quite sure that those figures will be interpreted as indicating that if we could only better the housing, we should at once lower the phthisis death-rate. Now, this is precisely one of the "difficulties of the subtle kind" which I want to illustrate. We have found association, we must not without full inquiry assert that it marks causation.

If mere association were evidence of causation, there is ample proof that back-to-back houses or one-apartment tenements produce high infantile death-rates and high phthisis rates. The Galton Eugenics Laboratory will show, in a forthcoming memoir, that in such houses more alcohol is consumed, their tenants are more irregular and less cleanly in their habits, their wages are lower and they pay less rent. I wish some keen medical officer would also tabulate the cases of insanity, epilepsy and imbecility in his district, with regard to the nature of the home in which the patient had been living, and tell us the result. There is no more important problem before both eugenist and medical officer than

whether man is the product of his environment, or the environment the product of the man. Do the physically and mentally inferior get lower wages and tend to drift towards the cheaper and dirtier type of house, or does the house make the occupants physically and mentally inferior? If neither statement is true, then what is the quantitative measure of their relative shares in the joint result?

Returning to our tuberculosis data, we have to ask three fundamental questions:

(i.) We know that the age distribution is immensely important in the case of tuberculosis. We ask are the age distributions of people living in tenements of 1, 2, 3, etc., apartments the same? Clearly, if persons of the susceptible ages live more in 1 or 2 apartments, the relation between smallness of house and prevalence of phthisis may be spurious. Now on the basis of the 1911 Census for Glasgow, which gives the number of persons of each age for each type of house, I have distributed 2,419 cases of phthisis notified in 1910 with their ages among the people living in the different types of houses in Glasgow.

I find as follows:

TABLE 8.—*Distribution of Cases of Phthisis in Glasgow.*

	Number of rooms.			
	1	2	3	4 and over.
General population, 1911	13·8 %	48·3 %	21·1 %	16·8 %
Expected phthisis distribution, allowing for age	13·4 %	46·3 %	21·8 %	18·5 %
Actual phthisis distribution	22·3 %	53·6 %	15·5 %	8·6 %

Clearly phthisis does not predominate in small houses because they are more occupied by persons of susceptible ages. Allowance for age makes little difference, although *a priori* this could *not* be assumed.*

(ii.) Our second problem is this: Do the persons who by occupation have greater liability to phthisis occupy in greater numbers smaller houses?

Now, it is well known that certain trades have high mortality rates. When I first came to investigate the matter I anticipated that trades of all sorts with a high death-rate, the true dangerous trades, would show a high rate of wages, but with few exceptions the exactly opposite is the case; there is quite a high correlation between the death-rate of a trade

* There is a possibility of fallacy even here, if the institutional cases are really th- *old* cases.

and the smallness of the wages paid in that trade. In other words, trades with high mortality are trades of low wages, and their followers will obviously be found in one and two-roomed houses. Here, again, we are on the vicious circle with all its subtle difficulties. Do physically inferior men follow "high mortality trades" because they can demand no higher wage, and thus make the trade a high mortality trade? Or, does the lower wage reduce the wage-receiver to a physically inferior state and thus make him susceptible to disease? The main point to be borne in mind is this: That in classes and families where there is no immediate question of hunger, general health varies enormously and is hereditary. There is no reason to suppose that general health and physique do not vary as largely in the working classes, or that, in the main, commercial economics do not regulate wages by fitness. In other words, we should expect to find the physically weaker members of our society receiving lower wages and occupying the poorer houses. That such persons are more susceptible to phthisis goes almost without saying.

Dr. Leslie Mackenzie and Captain Foster, in their valuable *Report on the Physical Condition of Children attending the Public Schools of Glasgow, 1907*, have divided the schools into four groups: the first, *A*, comprises the poorer districts, the second, *B*, the poor districts, the third, *C*, those of a better class, and the fourth, *D*, those of a still higher class. They state that "these groups indicate a real social gradation," and if we represent their homes in the same manner as we have just done those of the phthisical we have the following result:—

TABLE 9.—*Homes of School Children of Glasgow.*

	Number of rooms.			
	1	2	3	4 and over.
Grade D	0·6	22·0	39·8	37·7
Grade C	3·0	48·0	36·8	12·2
Grade B	7·4	63·9	24·1	4·6
Grade A	14·8	72·6	11·3	1·3
Phthisis	22·3	53·6	15·5	8·6

Now Grade A contains thirty-four per cent. of all the school children of Glasgow, and the phthisical have larger percentages of better homes than these Grade A children. It seems to me that the social grade of the phthisical patients deserves close consideration, and especially we ought to know their employment and wage before and after onset. How

far did they originally spring from a social grade occupying the poorer tenements? How far has that tenancy followed the onset of the disease? As suggestive in this matter we may note that Dr. Chalmers tells us that sixty-five per cent. of the total notifications were ultimately obtained through charitable or rate-aided sources, forty-four per cent. being from poor law sources alone. Further, in the Glasgow special inquiry forty-six per cent. of the adult phthisical males were unable to work.*

The instant we realise these points we see how urgent is the need for further investigation. It may quite well be that the predominance of one-roomed and two-roomed houses among the phthisical is the origin of the disease; but, on the other hand, the predominance of such houses may merely arise from the fact that sixty-five per cent. of the tuberculous are in receipt of charitable relief, or that forty-six per cent. of the adult males can do no work. These points will never be cleared up without far fuller knowledge than we have got yet of the *past* history of phthisical families. What was their original economic position and their trade?

Again this question of past history brings me to my third and last point:

(iii.) What is the past physical history of the stocks of these phthisical individuals? Are they economically weak because they are phthisical, or because they were originally of physically inferior stock? In the pedigrees of general degeneracy which we have collected in the Eugenics Laboratory (*i.e.*, pedigrees of epilepsy, insanity, mental defect, hare-lip, cataract and albinism, etc.) there is a marked excess of phthisis, and such families are as a rule economically as well as physically or mentally unfit. To what extent do they contribute to the phthisical contingent in the poorer homes? When you think over these points, I feel sure you must agree with me that the matter is much more subtle than we can plomb by merely comparing percentages of normal and phthisical homes of different sizes. We have to ask ourselves what is the relation between phthisis incidence and one or two-roomed homes for (i.) constant age distribution; (ii.) constant economic conditions antecedent to the onset; and (iii.) constant physique in the stock. We have to reach a "partial correlation."

I can conceive no more hopeful or profitable inquiry than the attempt to disentangle the multiple causes (trades or occupations, housing, heredity) which bear on the incidence of phthisis in one of our large towns. It would be relatively easy in relation to the system of notification-visits, but the schedules I have so far seen do not appear to cover adequately such a fundamental inquiry.

* Report, pp. 7 and 26.

At the risk of wearying you I must illustrate further these subtle difficulties of statistical interpretation. I take the following figures from a report published by the Home Office, they deal with female inebriates:—

TABLE 10.

Distribution of Fitness and Duration of Alcoholism in 885 Inebriates.

Conditions for Hard Work	Years addicted to drunkenness.						
	5 and under.	6—10	11—20	21—30	31—40	41—50	Totals.
Fit	96	216	214	38	7	—	571
Unfit	30	104	130	24	5	1	294
Totals	126	320	344	62	12	1	855
Percentages of Unfit	23·8	32·5	37·8	38·7	41·7	100	34·0

Now, can you conceive a better demonstration than this table that the longer drunkenness lasts the more physically unfit becomes the drinker? These numbers are so convincing that they will become a text wherever the evil of alcohol is preached!

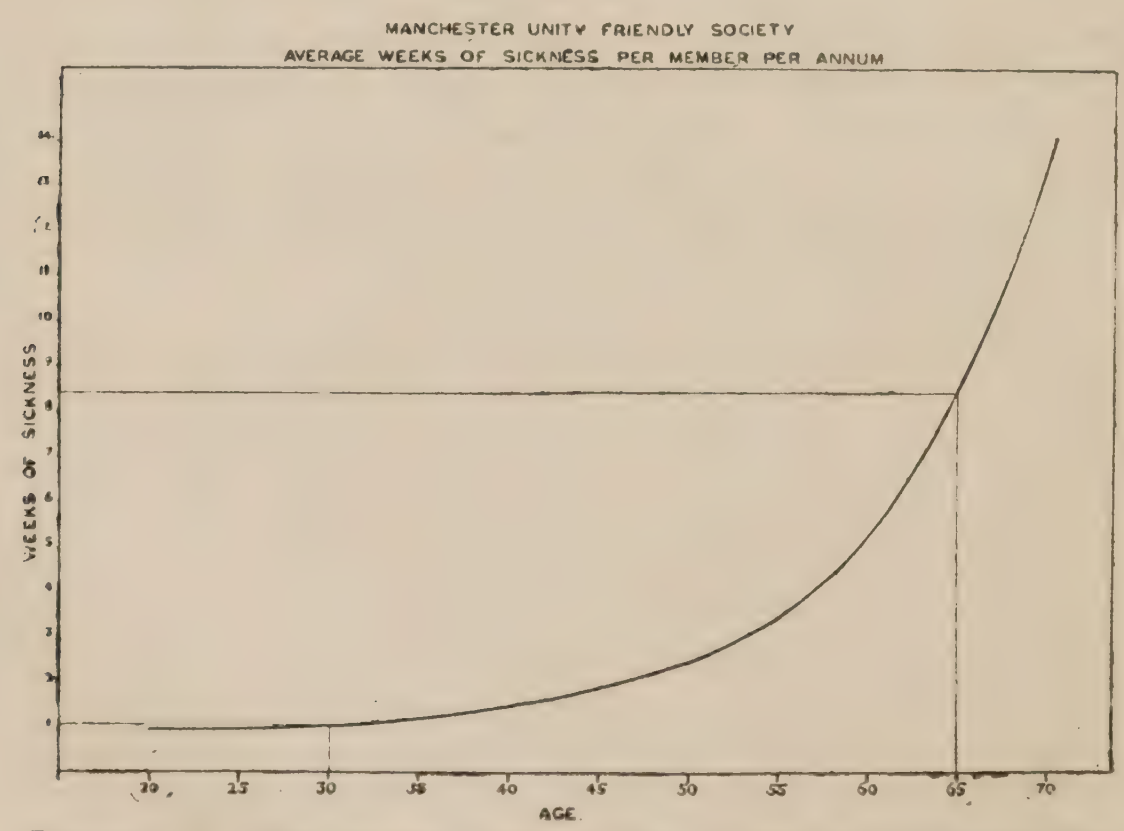

Diagram III.—This diagram indicates how "unfitness" increases with increasing age. At 65 the average number of weeks of sickness is more than 8 times that at 30.

The author of the Report tells us that "the figures also demonstrate the influence of continued drunkenness and the life associated with drunkenness in the making of the unfit. The definite and regular increase in the percentage of the unfit with every additional decade of habitual drunkenness is unmistakable."

The percentage of unfitness has nearly doubled in 30 to 40 years of drunkenness. Now please look at something of an entirely different character. Here is a diagram giving the weeks of sickness of a great Friendly Society for each year of age. You see at once that between 30 and 70 years of age the weeks of sickness are not merely doubled, they are multiplied by ten! The person who has had thirty to fifty years of drunkenness is, as a rule, thirty-five to forty years older than the person who has had only five years of such a life, and on that very account we must expect such persons to be less fit for hard work.

Clearly these statistics I have just shown you have no meaning at all until we have allowed for the feebleness of the increasing age of those who have had a longer duration of drinking. How are we to do this? Why, there is one simple and direct method: we may inquire whether among inebriate women of the *same age*, the unfit have had a longer duration of alcoholism than the fit. This is the method adopted by Dr. David Heron in his recently issued monograph on Extreme Alcoholism in Adults, and the results are very remarkable:—

TABLE 11.

Age of inebriates.	Mean duration of alcoholism.		Duration of fit less that of unfit.
	Among the fit.	Among the unfit.	
25–27	8·53	8·46	+ ·07
28–30	10·49	10·35	+ ·14
31–33	11·15	11·08	+ ·07
34–36	11·92	13·36	− 1·44
37–39	12·74	15·71	− 2·97
40–42	13·45	13·35	+ ·10
43–45	14·72	13·43	+ 1·29
46–48	13·45	14·25	− ·80
49–51	16·87	13·90	+ 2·97
52 and over	14·31	12·60	+ 1·71

For 7 out of 10 age groups the fit have been longer alcoholic than the unfit.

Whereas we should have expected to find of two women of the same age, one fit and the other unfit, the latter would have had the longer duration of alcoholism, yet actually there is little difference and the *fit* women, as shown by the last column, have on the whole rather the longer duration of alcoholism.

Now this illustration is of immense importance because it leads us directly to a most important principle: Two things like years of drunkenness and increasing unfitness may be obviously associated together and this association may lead us to sweeping conclusions, and yet the association be not a true causation. We may have neglected an important factor, in this case the increasing age of those who have been drinking the longer. Here we reach the great rule of modern statistics: "When investigating the relation of two characters which you find associated, test whether they still remain related after you have given all other characters likely to be influential constant values. Before you have done this you certainly must not treat the relation as a *causative* one."

The full theory of this method is what in modern statistics we term the treatment by partial correlation. We say that for constant age the partial correlation between years of drunkenness and physical fitness for hard work appears on the basis of the Home Office statistics of the inebriate reformatories to be zero.

Let us try to illustrate this important conception of partial correlation from other data which have immense importance from the standpoints both of eugenics and public health. My illustrations shall now be drawn from a field which is attracting much attention at the present time, namely that of infantile mortality. This field leads us at once to the questions of employment of mothers, of breast feeding, and of insanitary houses. These are the very points on which men propose largely to legislate for unrepresented women, and it certainly befits us men to be very sure of our premises. Does employment of mothers increase the infantile death-rate? Is there less artificial feeding where that death-rate is lower? Do back-to-back houses show higher infantile mortality than through houses? Now first let us look at breast feeding. I take data most kindly provided for me by Dr. Pilkington, Medical Officer of Health for Preston. We have:—

TABLE 12.—*Preston.*

Mortality Rate 1st year of life.		
Entirely breast-fed	13·2	per cent.
Partly or entirely artificially fed	15·7	„
Babies in bad health in first year.		
Entirely breast-fed	5·5	„
Partly or entirely artificially fed	11·7	„
Dead and delicate babies.		
Entirely breast-fed	18·7	„
Partly or entirely artificially fed	27·4	„

Could anything be more conclusive? We see that nearly 50 per cent. more babies die or are delicate when the mother does not entirely breast-feed. But Dr. Pilkington went further; he enquired into the reasons for entirely or partly artificially feeding. The reasons may be classed into two big groups, those cases for which it was needful either because the mother could not provide the milk, or because the infant could not take it, and those cases where the mothers gave up nursing optionally, because they wished to return to the factory or because they disliked nursing. Now let us examine the like rates when we distinguish these two classes. We have :—

TABLE 13.—*Preston.*

Mortality Rate, 1st year of life.	
Entirely breast-fed	13·2 per cent.
Optional artificial feeders	7·3 „
Necessary artificial feeders	20·5 „
Babies in bad health.	
Entirely breast-fed	5·5 „
Optional artificial feeders	7·3 „
Necessary artificial feeders	14·2 „
Dead and delicate babies.	
Entirely breast-fed	18·5 „
Optional artificial feeders	14·6 „
Necessary artificial feeders	34·7 „

It would appear from these results that in Preston, at any rate, it is not the artificial feeding *per se* which is the source of the high death and delicacy rates. The heavy rates arise solely in the case of those women who *necessarily* adopt artificial feeding, either because they cannot provide the food themselves, or because the baby is very delicate; the high rates arise because the artificial feeding category covers largely mothers who are themselves delicate or have delicate children.

It is well in such an important matter to give a control series. I am able to do this by the courtesy of Dr. Greenwood of Blackburn. We find from his data :

TABLE 14.—*Blackburn.*

Mortality Rate, 1st year of life.	
Entirely breast-fed	6·3 per cent.
Partly or entirely artificially fed	15·8 „

Babies in bad health.

Entirely breast-fed	24·0 per cent.	
Partly or entirely artificially fed	19·0 „	*

Dead and delicate babies.

Entirely breast-fed	30·3 „	
Partly or entirely artificially fed	34·8 „	

But when we analyse these returns in the previous manner we find :

TABLE 15.—*Blackburn.*

Mortality Rate, 1st year of life.

Entirely breast-fed	6·3 per cent.	
Optional artificial feeders	6·9 „	
Necessary artificial feeders	22·0 „	

Babies in bad health.

Entirely breast-fed	24·0 „	
Optional artificial feeders	17·2 „	*
Necessary artificial feeders	20·0 „	*

Dead and delicate babies.

Entirely breast-fed	30·3 „	
Optional artificial feeders	24·1 „	
Necessary artificial feeders	42·0 „	

These results absolutely confirm those from Preston, the death-rate of artificially fed babies is swollen because they include mothers *unable* to feed their children. These results suggest that it is not the artificial feeding, but the *health* of the mother which is the dominating factor in the mortality and delicacy of the infant.

I can bring home to you how very much more the health of the mother means than the nature of the feeding by considering the percentage of dead and unhealthy babies in relation to nature of feeding and health of mothers in Preston and Blackburn :

TABLE 16.—*Percentage of Dead and Delicate Babies at end of 1st Year.*

	Preston.	Diff.	Blackburn.	Diff.
Entirely breast-fed	18·7	8·7	30·3	4·5
Entirely or partly artificially fed ...	27·4		34·8	
Mother's health good	23·2	15·4	28·4	16·9
Mother's health bad	38 6		45·3	

* Illustrations of the selective death-rate in infantile mortality, a death-rate recently vigorously denied from this platform.

It will be seen that the difference of the rates in these two cases is of a wholly different order.

Let us turn now to the question of mothers' employment.

Miss E. M. Elderton, of the Eugenics Laboratory, finds very small relationships between infantile death-rate and the employment of mothers in a great variety of districts; the influence of employment is not as great as that of breast feeding, and sensibly less than that of mother's health.

For example taking Preston:—

TABLE 17.—*Employment of Mothers.*

PRESTON.	Neither before nor after.	Before and after.	Before only.
Death rate	17·7	17·2	18·5
Delicacy rate	9·1	9·9	5·3
Death and delicacy rate	26·8	27·1	23·8

Here we have employment making a difference of only about 3 in the death and delicacy rate, whereas we have already seen that in the same town the mother's health makes a difference of 15 in the same rate!

I ask you to look at the same thing from Blackburn:—

TABLE 18.—*Employment of Mothers.*

BLACKBURN.	Neither before nor after	Before and after.	Before only.
Death rate	10·6	13·9	18·9
Delicacy rate	22·0	20·0	21·1
Death and delicacy rate	32·6	33·9	40·0

Here we have the paradox that the mothers employed before and after have better babies than those employed before but not after. The real solution no doubt lies again in the health of the mothers, the mothers not returning after their confinement being on the average in weaker health. If we take all employed as against unemployed, we find the death and delicacy rates of the two classes are 32·6 and 35·6, and even this difference is incomparable with that of health which gives 28·4 and 45·3 for healthy and unhealthy mothers! How about unhealthy and healthy fathers, some of you may ask? Well, we have both reports on the father's health and on his occupation. The latter may be classified into skilled, factory and general labour groups. It is well known that the mortality rate and presumably, therefore, the health of general labourers is far below that of skilled workers and factory hands. The results for Blackburn are as follows:—

TABLE 19.—*Death and Delicacy Rates of Babies for Occupation of Father.*

Skilled Workers	31·1
Factory Hands	30·4
General Labourers	42·3

Employed Mothers ...	32·6	Unemployed Mothers ...	35·6

The occupation of the father is thus more influential than the employment of the mother, and if we legislate for the one, we must legislate for the other. Indeed, if we legislate for either of them we ought to legislate for the "baby-pacifier," which gives still more marked differences in the rates.

TABLE 20.—*Infantile Death-rate—Rochdale.*

General rate 1909	9·0
Rate after dummy teat stage	7·5

Mother not employed ...	8·0	Mother employed ...	12·3
Dummy teat not used ...	4·0	Dummy teat used ...	8·6

Percentage range in death-rate associated with mothers' employment ... 44
Percentage range in death-rate associated with dummy teat 61

Now turn to the health of the father. The accompanying table shows us the relative influence on death and delicacy rate of babies in the first year of life for Preston:—

TABLE 21.—*Death and Delicacy Rate for Infants.*
General Rate 1908, 25·6.

				Range.
Father's health good ...	24·4	Father's health bad ...	54·2*	29·8
Mother's health good ...	23·2	Mother's health bad ...	38·6	15·4
Father's labour high class	21·1	Father's labour low class	31·8	10·7
Entirely breast fed ...	18·7	Entirely or partly art. fed	27·4	8·7
Mother unemployed† ...	26·8	Mother employed† ...	27·11	0·3

You will I think agree, on examining this table, that parental health is fifty per cent. more important than parental occupation or breast feeding, and that all four are immensely more significant than the employment of women.

But every medical officer of health is surely bound to tell us that if he had money enough he could give us health? I hope it may be so, but the question is rather the measure of the gift he can provide us with.

* Excessive owing to smallness of numbers, but high at Blackburn also, where we have death and delicacy rate = 34·3, but for father's health good = 30·7, father's health bad 48·5, range 17·8.

† Before and after.

Has he anything like the same balance at his bank that Nature has to draw upon at hers?

Let me illustrate my point, and the pitfalls we may fall into by superficial reasoning. By the kindness of Dr. Anderson, of Rochdale, I have been able to see much material as to the effect of back-to-back or non-through houses. In something like 2,000 houses in Rochdale the infant mortality in the first year of life was 9·2 in the through-houses, and 12·7 in the non-through houses. Could any evidence better support that of Dr. L. W. D. Mair's Report to the Local Government Board on the evil of back-to-back houses? Why, the back-to-back house increases the death-rate by thirty-eight per cent.! Shall we, however, after what we have seen to-night, rest content with such a crude result? Shall not we ask whether the type of people living in through and in back-to-back houses are the same? Whether they start physically, mentally and economically on the same planes? Dr. Mair tried to make the people in the two types of houses of the same class, but he gives us no reliable estimate of their economic means or their habits, and he is obliged to confess that even after selecting his back-to-back houses he found a difference of 1s., or twenty per cent., in the average weekly rent. Without any selection of houses at all, we find in Rochdale every bad quality associated with the back-to-back house in most intimate degree: over-crowding, uncleanliness, poor means, irregularity of father's work, habits of parents. The difference of death-rate has no significance until these quantities have been equalised in back-to-back and through houses! Examine the following table:—

TABLE 22.—*Through and Not-through Houses.—Rochdale.*

	Through houses.	Not-through houses.
Infant mortality ...	9·2 per cent.	12·7 per cent.
Poor means of parents	66·8 „	99·1 „
Bad habits of parents	17·0 „	57·1 „

Till we have at least allowed for the poor means and the bad habits of parents in back-to-back houses, it is really idle to discuss the infantile mortality rate in such houses, But you may ask, how do I know that bad habits are so influential? That has also been provided by Dr. Anderson, and here it is:—

TABLE 23.—*Infant Mortality in Rochdale.*

General Rate ... 10·4 per cent.

	per cent.		per cent.	Range. per cent.
Through house	9·2	Not-through house	12·7	3·5
Parents of good habits	7·6	Parents of bad habits	16·3	8·9

We see that even if the back-to-back house be partly the source of the higher infantile death-rate, the medical officer of health who could alter parental habits would achieve more than twice as much as he who carried a bye-law against back-to-back houses. But what if the higher death-rate in back-to-back houses is really due to the predominance of parents of bad habits and bad physique in the cheaper houses? Is not this cheaper house exactly where we should expect to find them, and shall we gain anything socially by transferring them to through houses? The test question is: What infantile mortality have parents of good habits in back-to-back houses? This is answered in the next table.

TABLE 24.—*Influence of Back-to-back Houses and Parental Habits on Infantile Death-rate. Rochdale, 1910.*

	Through houses. All parents.		Not-through houses. All parents.	
At six months	6·9		9·9	
At a year	9·2		12·7	
	Parents with:		Parents with:	
	Good habits.	Bad habits.	Good habits.	Bad habits.
At six months	5·8	12·4	6·3	12·7
At a year	7·6	16·8	7·7	16·1

Percentage of parents with bad habits: Through houses, 17·0; not-through houses, 57·1.

This shows us that in Rochdale at any rate it is the character of the parent and not of the house which kills the child.

By the courtesy of the medical officer of health for Sheffield, I have received data from which I am able fully to confirm these Rochdale results.

TABLE 25.—*Sheffield: Death and Delicacy rates for Female Babies (circa 4,000).*

Character.	Rates.				Range.
Health of Mother	Good	6·4	Bad	14·6	8·2
Health of Father	Good	7·8	Bad	13·0	5·2
Habits of Father	Good	7·5	Bad	12·5	5·0
Habits of Mother	Good	7·4	Bad	10 9	3·5
Employment of Mother	Not employed	7·9	Employed	8·5	0·6
Type of House	Through	7·7	Back-to-back	8·3	0·6

Thus the health and habits of *both* parents are far more influential than employment of mother or type of house.

Such results force me to believe that as the Public Health Service places more and more data at the service of social investigators, we shall realise more and more how man moulds his circumstances and not mere environment moulds the human herd. Take this very question of health. I have tried to investigate its distribution and variation in the professional classes, where on the whole the cruder forms of environment have little influence, and here how markedly, considering the many extraneous sources of bad health, do we find it hereditary! I would ask you to look at the diagrams which demonstrate this. The one exception appears to be the offspring of very delicate parents, and the probable

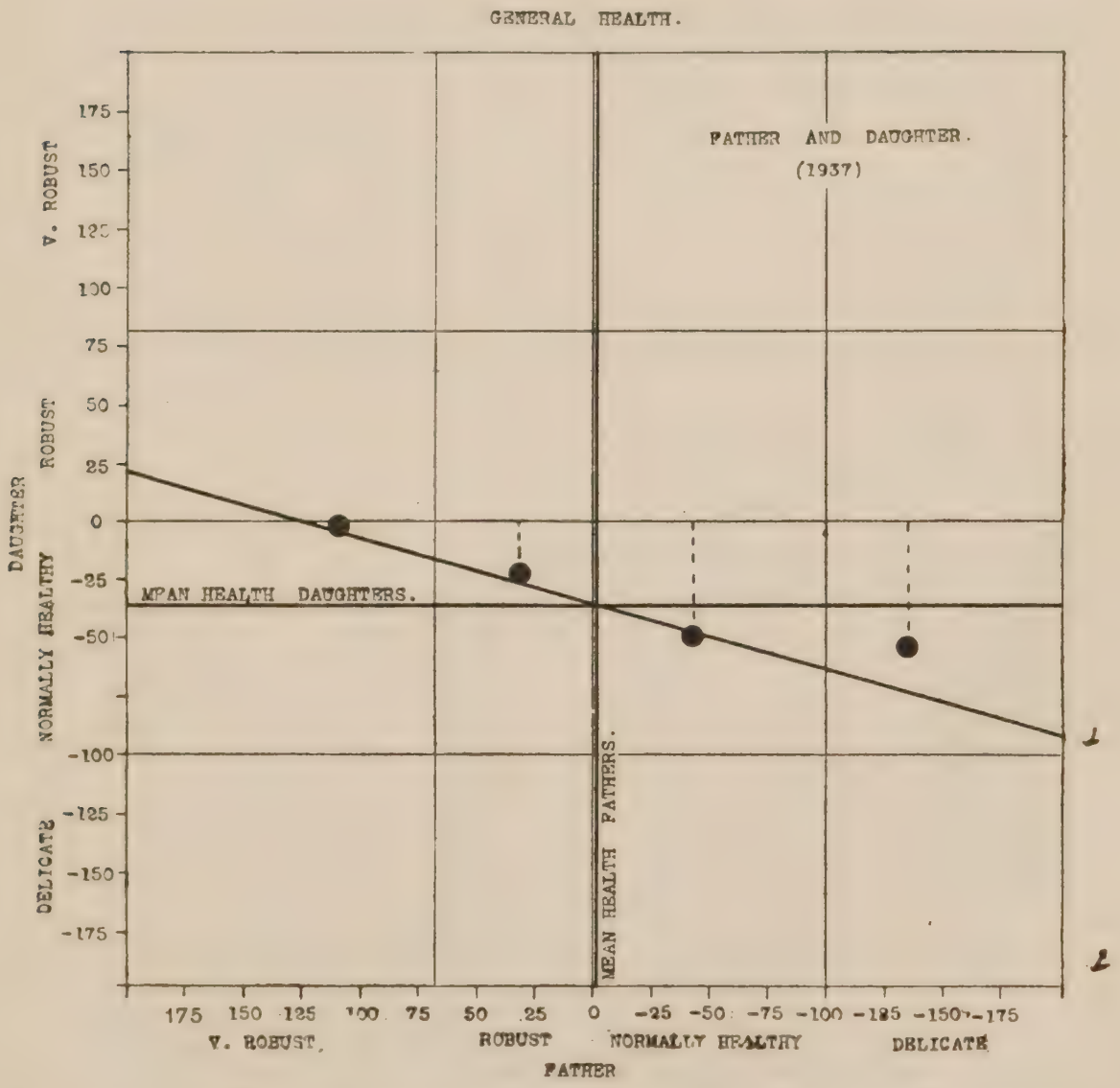

Diagram IV.—Showing the heredity of General Health in the Professional Classes. 1937 cases of father and daughter. Increasing delicacy of father marks increasing delicacy of daughter. Note the anomaly in the case of delicate fathers.

reason for this is shewn in my last diagram which indicates why the delicate children of delicate parents are not recorded in their due proportions: they die before their health can be recorded in larger proportions than the children of healthier parents. I once remarked to a coastguardsman: "What a healthy spot for your bairns!" "Well," he said in reply, "I says of children some is un'ealthy wherever you puts 'em, and the others ag'en is 'ealthy no matter where they lives."

But if, as I have endeavoured to show you to-night, health is a real hereditary character and the health of the parents is far more important than the question of back-to-back houses, one-apartment tenements, the

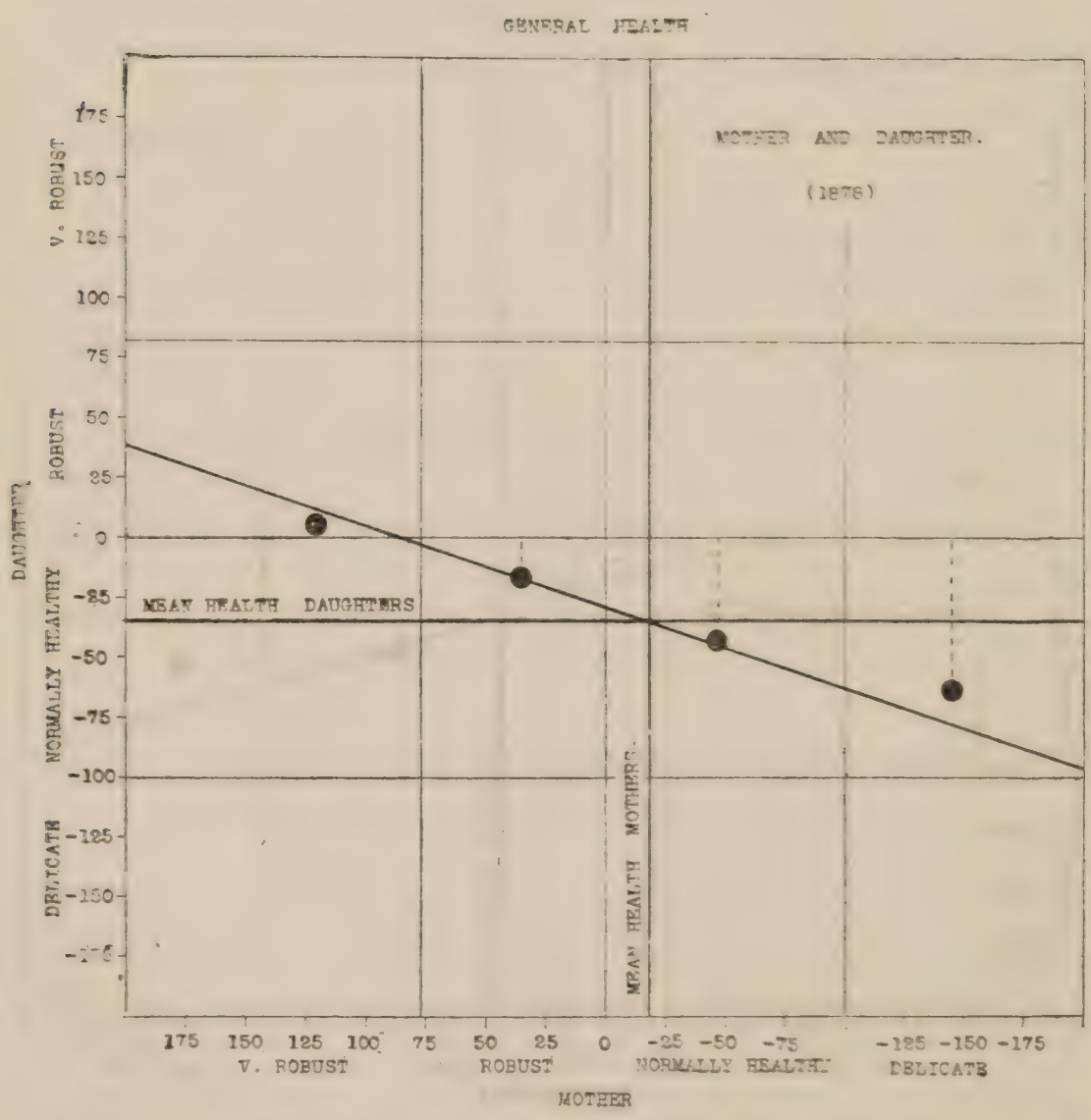

Diagram V.—Showing the heredity of General Health in the Professional Classes. 1878 cases of mother and daughter. Increasing delicacy of mother marks increasing delicacy of daughter. Note the anomaly in the case of delicate mothers.

employment of mothers or breast feeding, what function, some of you may ask, am I leaving to the Public Health Service? Well, Ladies and Gentlemen, when I say that you cannot produce brains by multiplying universities and technical colleges, am I arguing that such institutions are of no service at all? If brains are bred, not manufactured by colleges, are we to neglect the fact that, although a keen razor can never be made of bad steel, a good steel requires setting and tempering before it can fitly perform its functions? Because I believe that health in the main is the product of sound stock, and that you cannot produce it by changing the habitations of unsound stock, is it not possible for me also to hold that

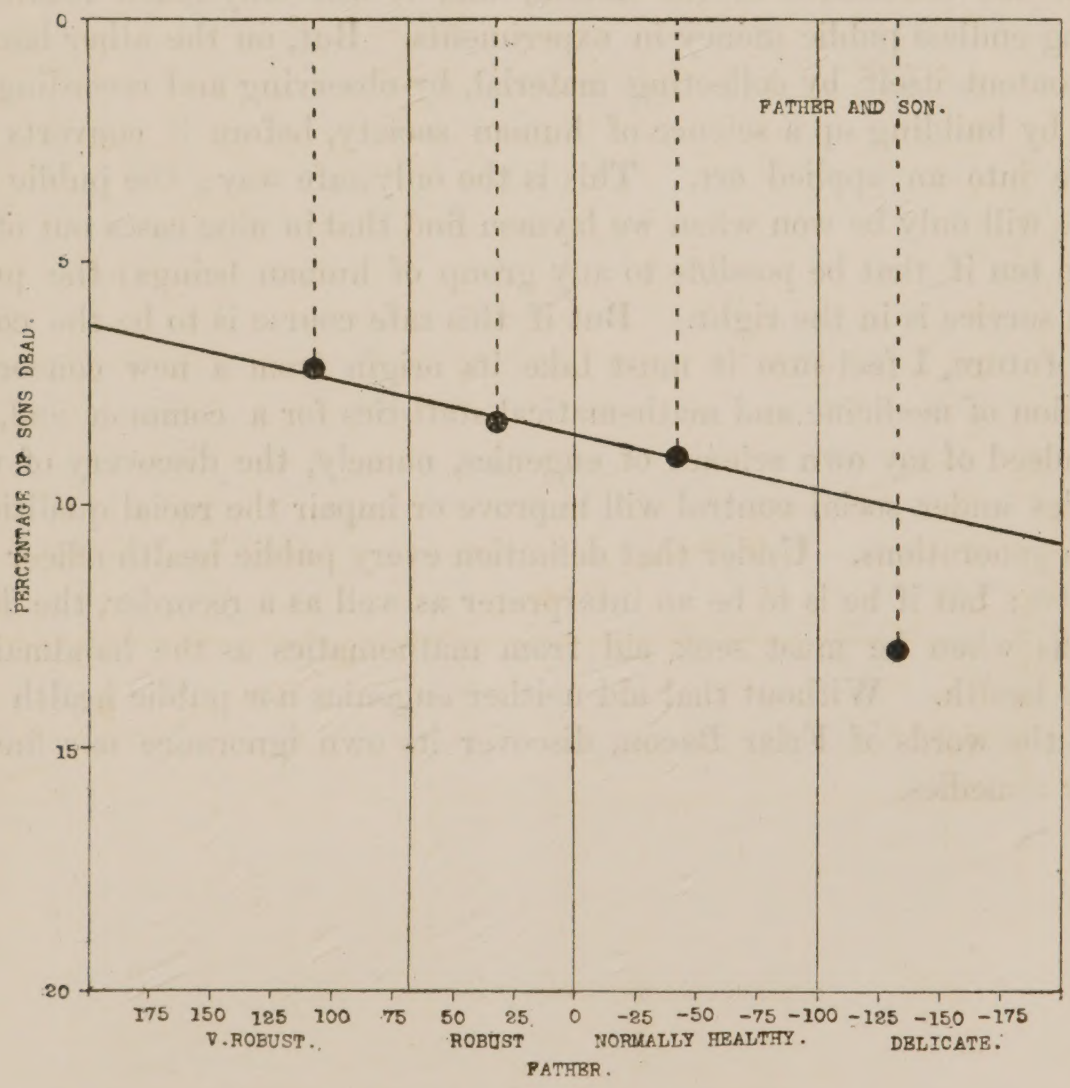

Diagram VI.—To explain anomaly in offspring of delicate parents. Percentage of dead sons to various classes of fathers. The delicate fathers have more dead sons and so a larger proportion of their sons escape the record. Only the healthy offspring of delicate parents survive to be recorded.

increased sanitary efficiency will render the work and life of the healthy still fuller and more effective?

Meanwhile, every medical officer in this country, who realises the enormous power of gaining knowledge which his position gives him, can aid in building up a science upon which a sure foundation for race fitness can be constructed. Only let us not be hasty, let us not rush upon the basis of most superficial statistics to conclusions from which still more superficial legislation will flow. We are at the parting of the ways now. The public health service of this country is bound to develop marvellously in the next fifty years. It may, overlooking the infinite subtlety of these social problems; proceed experimentally before it has fully inquired and attained to adequate knowledge. In that case it is bound sooner or later to lose the confidence of the nation, and it will only reach results by wasting endless public money in experiments. But, on the other hand, it may content itself by collecting material, by observing and recording; in short, by building up a science of human society, before it converts that science into an applied art. This is the only safe way; the public confidence will only be won when we laymen find that in nine cases out of ten (ten in ten if that be possible to any group of human beings) the public health service is in the right. But if this safe course is to be the course of the future, I feel sure it must take its origin from a new concordat: the union of medicine and mathematical statistics for a common end, the end indeed of my own science of eugenics, namely, the discovery of what agencies under social control will improve or impair the racial qualities of future generations. Under that definition every public health officer is a eugenist; but if he is to be an interpreter as well as a recorder, the day is with us when he must seek aid from mathematics as the handmaid of public health. Without that aid neither eugenics nor public health can, to use the words of Friar Bacon, discover its own ignorance nor find its proper remedies.

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